trary meanings. Biologists say they understand one another, and therefore I suppose they do; but I wish, in pity, they would enlighten me. Why do Lamarckians and neo-Darwinians say "inherit" when they mean "vary"? Why do Mendelians and biometricians say "inherit" when they mean "reproduce"? Meanwhile, I cannot help suspecting that something is wrong. Consider what has happened—Lamarck's theory and half a century of stasis; Darwin's brilliant lucidity and twenty years of progress, with biology in its splendour, a great intellectual force; Weismann's effort, and nearly half a century of controversy, with interest in the subject limited to some (not all) zoologists and botanists, and of these few a majority resentful of trespassers.

I propose in two or three letters to adopt the physiological classification when dealing with three or four biological subjects. Biologists, I hope, will be tolerant towards one who uses this classification because, admittedly, he does not understand the difficult language they speak. G. ARCHDALL REID.

9 Victoria Road South, Southsea, January 16.

Atmospheric Refraction.

DR. BALL is surely wrong in suggesting in NATURE of January 5, p. 8, that the difference between Mr. Mallock's figure for the radius of curvature of a nearly horizontal ray and that given by Dr. de Graaff Hunter is accounted for by any consideration of the curvature of the wave-front. If such were the case, then an observer looking towards the sea horizon would see a ray of light in different directions for different initial curvatures of the wave-front. Suppose an observer from the bridge of a ship were looking at a searchlight placed at sea-level at the extreme limit of visibility. The rays of the searchlight beam would be plane waves, those coming from the barrel of the searchlight spherical. Does Dr. Ball wish us to infer that in such circumstances the visible beam would appear to the observer to issue from a point *above* the projector?—for that is what his suggestion leads to.

To my mind, a great deal of the confusion between refraction figures given by different authorities lies in their attempt to connect refraction with variations of temperature before they have properly considered the subject from the point of view of variations in refractive index. If we assume that, over the sea at all events, the refractive index stratification is one which is spherical and concentric with the earth, then the general equation of any ray of light is

$$pn = \text{constant},$$

where n is the refractive index and p the perpendicular upon the tangent to the ray from the earth's centre (see Herman, "Geometrical Optics," p. 305, or Heath, "Geometrical Optics," p. 329).

If r is the distance of any point upon the ray from the earth's centre, h the height of the point above the earth's surface, and R the earth's radius, then $r = \mathrm{R} + h$.

Now n must be some function of the height $= f(h) = f(r - \mathrm{R})$, and hence the "$p$, r" equation of the ray is

$$pf(r - \mathrm{R}) = \text{constant} = \mathrm{C}.$$

The radius of curvature of the ray is thus

$$\sigma = r\frac{dr}{dp},$$

$$= -\frac{r\{f(r-\mathrm{R})\}^2}{\mathrm{C}}\bigg/\frac{df}{dr},$$

or

$$= -\frac{rn^2}{\mathrm{C}}\bigg/\frac{dn}{dh}.$$

As we are dealing with a ray which is nearly horizontal, variations in r and n^2 cannot have large effects upon σ. The variations in r might amount to 1 part in 200,000 if the ray never gets above 100 ft. above the surface of the sea; the refractive index, which at the sea-level is 1·00029, could scarcely be reduced below 1·00027 in the same height, so that variations in n^2 could not exceed 4 parts in 100,000. It follows that the curvature of such rays is essentially proportional to the refractive index gradient. Since by Dale and Gladstone's law $n - 1$ is proportional to ρ, the density, the curvature of the ray-path becomes immediately proportional to the density-gradient. If we attempt to translate density-gradient into temperature-gradient, I see no means of doing so other than by making the assumption that the atmosphere is statically in equilibrium, in which case the formulæ given in my letter in NATURE of January 5 result immediately. But I have the gravest doubts of the legitimacy of such an assumption for the lower levels of the air. A steady motion leading to a dynamical relationship between pressure, density, and temperature is much more likely, but is, from the mathematician's point of view, a hopeless thing to try to set down owing to the impossibility of dealing with all the factors of the problem, such as rate of radiation of heat-energy from the earth or sea, rate of thermal conduction in the air, nature of the upward air-currents, and so on.

If however, we leave all such considerations aside and deal only with the established connections between curvature of the ray-path and the density-gradient, then we can only admit uniform curvature if we are prepared to admit that the density of the air in its lower levels is a linear function of the height. To such an admission I take the strongest exception. It is quite insufficient to account for a refraction of the visible sea horizon above the true horizontal—a phenomenon which, as every seaman knows, is by no means uncommon. T. Y. BAKER.

Admiralty Research Laboratory, Teddington,
Middlesex, January 7.

The Colours of Tempered Steel.

THE well-known and characteristic tints that appear on the surface of a tarnishable metal when it is heated in contact with air have been usually regarded as interference colours due to the formation of a thin film of oxide on the surface of the metal. The correctness of this explanation has, however, recently been questioned (A. Mallock, Proc. Roy. Soc., 1918), and rightly so, as a continuous film on a strongly reflected surface cannot on optical principles be expected to exhibit such vivid colours as those observed.

I have recently made some observations which shed a new light on this subject. It is found that the *missing colours* complementary to the tints seen by reflected light appear as light *scattered* or *diffracted* from the surface of the metal. In other words, if a plate of blue-tempered steel be held in a beam of light and viewed in such a direction that the regularly reflected light does not reach the eye, the metal shows a straw-yellow colour, and not the usual blue. It will be understood that the scattered light, being distributed over a large solid angle, appears much feebler than the regularly reflected colour, and in order to observe the effect satisfactorily the metal should have a smoothly polished surface before being heated up. Scratches and other irregularities show the ordinary colour of the film, and not the complementary tint. The most attractive effects are those exhibited by a heated copper plate, both on account of the vividness

© 1922 Nature Publishing Group

of the colours and on account of the ease with which the surface can be given a satisfactory polish.

It is clear from the observations mentioned above that the colours under discussion are in the nature of *diffraction effects* arising from a film which is not continuous, but has a close-grained structure. Interesting effects are observed when the surface of the illuminated plate is viewed through a nicol, the colour and intensity of the scattered, as well as of the regularly reflected, beams varying as the nicol is rotated about its axis. The most striking effect is obtained when the direction of observation is nearly parallel to the surface of the plate. The scattered light in this case is nearly completely polarised, and the colour of the regularly reflected light changes nearly to its complementary when the nicol is turned through 90°. The phenomena strongly recall to mind the observations of R. W. Wood on the colours of a frilled collodion film on a silvered surface, which have been discussed by the late Lord Rayleigh (*Phil. Mag.*, November, 1917), and it seems probable that the explanation of the phenomena will ultimately be found to be somewhat similar in the two cases.

C. V. RAMAN.

210 Bowbazaar Street, Calcutta, India,
October 11.

MR. MALLOCK has shown that the colour of the oxide film is an *intrinsic* property of the material of which it is composed and the material retains this property as it is gradually ground down from its original thickness to the vanishing point. Sir George Beilby's observations have confirmed this, and have further shown that the film is an aggregate *in open formation* through which oxygen molecules can penetrate to the metallic surface. For each temperature above the tempering range the thickness of the film is determined by the porosity of the aggregate to the oxygen molecules at that temperature. Direct experimental observations have shown the part played by *time of heating* at any given temperature. For example, at 275° C. a deep purple was reached in ten minutes, and this changed to blue from the margin inwards during a further period of twenty minutes. It was thus shown that the watchspring-blue, which could immediately be produced by a temperature of 300° C., could also be produced by heating at 275° for thirty minutes. Sir George Beilby's view is that the *intrinsic* colours of the films which are produced at different temperatures result from changes in molecular aggregation in relatively open formation of a similar nature to those which have been shown to occur in thin metal films, *e.g.* gold. This is referred to in his recently published volume entitled "Aggregation and Flow of Solids," sections 3 and 10.—ED. NATURE.

Some Terrestrial Experiments on Gravitation and Einstein's Theory.

THE object of this letter is to direct the attention of writers on Einstein's theory of gravitation to some recent experiments on the terrestrial aspects of gravitation which seem to have been overlooked, although they appear to be of great importance for the purpose of forming a just estimate of the correctness of Einstein's theory.

The first investigation referred to is that of Dr. P. E. Shaw on the effect of temperature on gravitative force (Phil. Trans., 1916, A, vol. 216, pp. 349–92). On p. 390 Dr. Shaw writes:—"When a large mass attracts a small one, the gravitative force between them increases by about 1/500 as the temperature of the large mass rises from, say, 15° C. to 215° C." The only cause capable of producing this effect on the relativity theory seems to me to be the absorption of heat by the large mass (lead), amounting to 6 calories, or $2{\cdot}5\times10^{8}$ ergs per gram, and resulting in a fractional increase of inertial mass of about $2{\cdot}8\times10^{-13}$. We require 7000 million times this amount in order to account for Dr. Shaw's result on the hypothesis of the proportionality of the gravitative and inertial masses, which is one of the basal assumptions of Einstein's theory.

Another investigation is that of Majorana on the absorption of the gravitational flux (*Phil. Mag.*, vol. 6, No. 39, pp. 488–504, 1920), in which he finds, *inter alia*, that a lead ball weighed *in vacuo* loses $7{\cdot}7\times10^{-10}$ of its weight when it is surrounded symmetrically by 104 kg. of mercury. If the gravitational flux be assumed to be absorbed by the mercury according to an exponential law of density and thickness, the *quenching constant*, or factor of absorption, is found to be $6{\cdot}73\times10^{-13}$ per unit density and length. A possible interpretation is that the gravitational mass of a homogeneous sphere at an outside point is only a fraction of its inertial mass; according to Majorana, it is about one-third for the sun. If this interpretation be legitimate, the results of Majorana, like those of Shaw, lead to the conclusion that the gravitational and inertial masses are not proportional to one another in all circumstances.

G. A. SCHOTT.

University College of Wales, Aberystwyth,
January 5.

British Scientific Instruments.

IN the timely and encouraging leader in NATURE of January 19, with which my experience is in entire agreement, there is a point of some importance to which reference is omitted. This is the practical question of cost. I would ask permission to draw a moral for application at the present time. Without expressing any opinion as to whether this cost could be reduced by improved methods of manufacture, I would direct attention to the fact that in the impoverished state of the finances of universities and similar bodies it is impossible adequately to equip their laboratories with costly apparatus.

The moral is this: *The most effective way in which Government intervention can assist British makers of scientific apparatus is to increase the grants to universities and to research in general.* It is impossible for individual workers to purchase expensive British instruments out of their own incomes, and until the resources of the laboratories in which they work are sufficiently increased it is an unjustifiable and foolish restriction to prevent their obtaining from abroad apparatus often admittedly inferior, but capable of good use. How many laboratories can afford to obtain Hilger optical apparatus or the Cambridge string galvanometer? It is further to be remembered that as science advances the instrumental equipment for continued pushing forward tends to become more elaborate, sensitive, and accurate, and necessarily of greater cost

W. M. BAYLISS.

University College, London.

Globular Lightning Discharge.

THE following is an account of what appears to have been a genuine case of globular electrical discharge observed by the sisters of one of my colleagues, the Misses Pitman, at Eastbourne on August 17 last. Authentic instances of this phenomenon are rare, and as the conditions which accompanied this particular

© 1922 Nature Publishing Group